knowledge of the psychology of brutes, seeing that we cannot directly interrogate them upon the nature of their feelings or mental states. The discussion which followed appears to have had the effect of somewhat modifying his original views; for these, as now stated in his book, are not so severely sceptical as they were when stated in these columns. That is to say, he now appears to recognise the possibility of comparative psychology as a science, although its subject-matter is necessarily restricted by the inadequacy of our "ejective" knowledge of animal intelligence.

We are in such full agreement with the whole essay that our only criticisms upon it refer to matters of comparative detail. These are as follows:—

Mr. Morgan gives it as his opinion that we cannot conceive of matter apart from motion (p. 94), for, in order to do so, we should require to conceive of matter as absolutely cold, "and of such absolutely cold matter we have no knowledge." The fact, however, that we have no knowledge of absolutely cold matter is no proof that we are not able to conceive of matter as absolutely cold. The so-called absolute zero of temperature surely admits of conception as definite as it would were it possible to take an actual reading of its occurrence.

Mr. Morgan's use of the word "instinct" appears to us equivocal. At one time instinctive actions are expressly affirmed to mean adaptive actions of an involuntary and unconscious kind (pp. 226-7); while at another time it is said "Mr. Darwin clearly shows that the satisfaction of any instinctive emotion carries with it a subdued form of pleasure; while, on the other hand, if those instinctive emotions be not satisfied, there results a still more marked feeling of uneasiness, which is a subdued form of pain" (p. 259). Now, clearly, there can be no such thing as an unconscious emotion, an unconscious form of pleasure, or a "still more marked [unconscious] feeling of uneasiness." Mr. Morgan thus appears to have fallen into the inevitable confusion which is the fate of all writers who fail clearly to distinguish between instinct and reflex action, or expressly to include the former term within the territory of consciousness. For these reasons we cannot follow the author's analysis where it leads up to the conclusion that volition is coextensive with consciousness (p. 226 *et seq.*). We may be conscious of the sudden anguish of neuralgia: can it be said that this consciousness is due to, or accompanied by, any act of volition? Mr. Morgan would answer that with the pain there arises a desire that it should cease (p. 229). But, in the first place, a desire is not a volition; and, in the next place, even the desire has here no time to arise before the pain is past.

In one place where Mr. Morgan refers to the views of the present writer, he represents them as differing from those of Dr. Bain, while in reality no difference obtains. First, he quotes the following passage from "Mental Evolution in Animals":—

"What is the difference between the mode of operation of the cerebral hemispheres and that of the lower ganglia which may be taken to correspond with the great subjective distinction between the consciousness which may attend the former, and the no-consciousness which is invariably characteristic of the latter? I think the only difference that can be pointed to is a difference of rate or time, which clearly implies that the nervous mechanism concerned has not been fully habituated to the performance of the response required. . . . Reflex action may be regarded as the rapid movement of a well-oiled machine, consciousness as the heat evolved by the internal friction of some other machine, and psychical processes as the light which is given out when such heat rises to redness. Consciousness is but an adjunct which arises when the physical process, owing to infrequency of repetition, complexity of operation, or other causes, involves what I have before called ganglionic friction."

Now, on this passage Mr. Morgan remarks that he does not consider such ganglionic friction so important a factor in the evolution of consciousness as is "the diffusion of nerve-disturbance" enunciated by Dr. Bain. But surely the former principle includes the latter. For it is only due to this internal friction that the diffusion of nerve-disturbance can be supposed to take place. If all the paths of nervous discharge were freely open, the nervous disturbance would course rapidly and easily along the habitual channels, with comparatively little diffusion as a result. It is only in cases where no one set of paths are more readily open than other sets that alternative directions are offered to the flow of nervous disturbance, with diffusion as a result. The resistances thus encountered—or the ganglionic friction thus created—finds its measurable expression in the delay of eventual response. But although ganglionic friction may arise from such "complexity of operation" (so leading to diffusion), it may also arise from "infrequency of repetition or other causes." Therefore the term ganglionic friction includes all that is expressed by the term diffusion, and differs from it only in being more comprehensive, or in recognising other conditions of cerebral action leading to consciousness, the occurrence of which is always expressed by delay.

GEORGE J. ROMANES

OUR BOOK SHELF

Spectrum Analysis. Six Lectures delivered in 1868 *before the Society of Apothecaries in London.* By Sir Henry E. Roscoe, F.R.S. Fourth Edition, Revised and Considerably Enlarged by the Author and by Arthur Schuster, Ph.D., F.R.S. (London: Macmillan and Co., 1886.)

THIS is a fourth edition of a well-known book, and the joint authors have evidently taken some trouble to bring the present edition up to date. To this end, the arrangement of the book, which is rather peculiar, lends itself very well. The peculiarity of the arrangement to which we refer is this. At the time that the lectures were first delivered, now nearly twenty years ago, the literature of the subject was so restricted that Prof. Roscoe found it easy and convenient to reinforce the subject-matter of each lecture by reprinting, immediately after it, the particular memoirs on which it had been based. Hence the first edition was a very precious boon to two classes of people: there was an excellent popular account of the new science, and there were the complete memoirs conveniently brought together for those who wished to go more deeply into the subject.

In the present edition an attempt has been made, as we have said, to bring the lectures more or less up to date, and considering the volume of the work which has been done since 1868, one can understand that this has been no easy task. When we pass, however, from the lectures to the appended memoirs so much cannot be said; indeed the interest of this part of the book is now chiefly antiquarian, if we except reprints of Dr. Schuster's own papers, which are given, we believe, *in extenso*,

while Prof. Young's observations on the sun, now fifteen years old, is the latest information we get in the appendixes on any solar matter, English and foreign work being ignored with a magnificent impartiality. In the same manner Vogel's work on the spectra of stars, the most extensive which has been accomplished by any one single individual up to this time, is also passed over, as is also Birmingham's work on the red stars.

We give these as instances of the treatment adopted. No doubt, had the initial idea of the book been carried out in its entirety by the insertion of the most important parts of these memoirs, the size of the volume would have been greatly increased, and this perhaps may be one reason for the violently selective treatment adopted; but it may be urged on the other hand that the value of the book would have been increased much more than its size, and further, that space might easily have been gained for some of the best modern work by the omission of those papers which, as we have said before, are now purely of antiquarian interest.

There was one feature in the third edition which we also regret very much to see dropped in the present one. This was a bibliography running over twenty pages, in perhaps its most convenient form, namely, a list of authors and a complete reference to their memoirs, arranged under the larger groupings of the subject-matter.

Trigonometry for Beginners, as far as the Solution of Triangles. By the Rev. J. B. Lock, M.A., Senior Fellow of Gonville and Caius College. (London: Macmillan and Co., 1886.)

THIS book covers exactly the same ground as Pinkerton's, which we noticed in NATURE, vol. xxxi. p. 148. The two have many good points in common, and we should be well satisfied to use either of them as a text-book. Mr. Lock's great advantage is preceptorial skill in arrangement and exposition. On this score he deserves much credit indeed. There are very few points on which it is possible to suggest improvement. The retention of the expression "circular measure" in all its former importance, notwithstanding the introduction and constant use of the term "radian," is regrettable but not of much consequence: the mode, however, which he employs for indicating the word "radian," *e.g.* writing π^c for π radians, is most unfortunate, and we should hope altogether unacceptable. It is surprising too to find so skilled a teacher following the multitude in condescending to recognise those unnecessary nuisances, "tabular logarithmic sines," &c. Their existence, Mr. Lock says, is due to a typographic difficulty—a statement we hesitate to give assent to; but, be their history what it may, they serve no purpose nowadays whatever, except to roughen the learner's path. Writers require to give them a foolish name and a special symbol, to alter the formulæ for solution, and to burden the learner with additional cautions,—and all for less than nothing. It seems almost malicious indeed to force on a "beginner" such gratuitous absurdities as "natural sines," "logarithmic sines," and "tabular logarithmic sines," when the entities to be dealt with are simply *sines* and *logarithms of sines*. If Mr. Lock in a succeeding edition could see his way to inaugurate the necessary reform here, many teachers would be grateful to him.

The Apparent Movements of the Planets and the Principal Astronomical Phenomena for the Year 1886. Illustrated with Charts showing the Paths of the Eleven Principal Planets among the Stars. By William Peck, F.R.A.S. (Edinburgh: Archibald and Peck, 1886)

BEGINNERS in astronomy will find this little compilation useful. Just the kind of information is brought together in it which persons interested, though not learned, in celestial phenomena want to be supplied with Technical language, too, is as much as possible avoided, while sufficient exactness for the purpose in view is usually preserved. Not, however, invariably; the statements regarding the two solar eclipses visible in 1886 are so loose as to be misleading. Eleven miniature maps, showing the paths through the constellations during the present year of seven primary and four minor planets, are neatly executed, and ought to prove acceptable to casual observers. Exception must be taken to the introductory assertion that Copernicus swept away all the "complicated machinery of the heavens." His reform of the Ptolemaic system was by no means so complete as Mr. Peck's expression implies. The retention by the Frauenburg astronomer of the old hypothesis of equable circular motion necessitated, in fact, the employment still of no less than thirty-four circles, by which to make plain, as he said, "the entire structure of the heavens"—that is, the revolutions of the moon and of the six known planets.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

An Earthquake Invention

IN NATURE of July 2 last, p. 213, I was accused by Prof. Piazzi-Smyth and Mr. D. A. Stevenson of having attempted to appropriate an invention of Mr. David Stevenson. The invention referred to was the joint introduced by Mr. Stevenson beneath the lamp-tables in certain lighthouses in this country.

In my reply (NATURE, vol. xxxii. p. 573) I pointed out the fact that the aseismatic joint had been independently invented by several investigators of earthquake phenomena, and so far as I was aware Mr. Mallet had appeared to have the prior claim to this invention. My reason for attributing the invention to Mr. Mallet is that when speaking of Japanese lighthouses he says: "I was consulted by Mr. Stevenson as to the general principles to be observed, and these edifices have been constructed so that they are presumedly proof against the most violent shocks likely to visit Japan; not perhaps upon the best possible plan, but upon such as is truly based upon the principles I have developed" (Palmieri's "Vesuvius," p. 43). As the aseismatic joints were portions of the lighthouses especially designed to render them proof against earthquakes, I naturally assumed that Mr. Mallet might be the first inventor of the ball-and-plate joint.

The only occasion on which I have posed as *the* author of the aseismatic joint in question, was when Messrs. Stevenson and Smyth promoted me to that *quasi*-enviable position.

Had these gentlemen recognised the fact that they were only reading a *brief note* about ball-and-plate joints, intercalated in a collection of notes on other subjects, and had they been well acquainted with the recent literature relating to aseismatic tables, they would certainly have refrained from the objectionable accusations made on July 2.

On *more than one* occasion I have referred to Mr. Stevenson's work in Japan. As an example of such a reference, Messrs. Stevenson and Smyth may turn to the *Times* of May 26—*a date which it will be observed is prior to the date of their unwarrantable attack.* In that paper there is a long letter on "Buildings and Earthquakes" signed with my name. When speaking of my house on shot, I there say, "*This experiment was very similar to one carried out by Mr. David Stevenson with regard to the lamp-tables in several of the lighthouses on the coast of Japan.* For several reasons, among which were the movements produced by wind, I abandoned the balls, and now have my house resting at each of its piers upon a handful of cast-iron shot. These shot, which are about the size of buckshot, have so increased the frictional resistance to rolling, that the house is practically astatic, and the motion in the house is in most earthquakes only about one-tenth of what it is outside."

I make especial reference to the *Times*, first because it is a